on Roberts' photograph (1889); the circular spots are stars, recognisable in the drawing. Unless this part of the Trouvelot drawing—the excellence of which is stated by Dr. Roberts himself—be very incorrect, the nebula would seem to have turned about 15° from left to right. The globular nebula (M. 32) to the other side of M. 31, seems also to have slightly shifted its position.

Evidence of the reality of such changes is of course only obtainable by comparing three or more photographs taken at comparatively wide intervals. In the meantime, this short notice in NATURE may call the attention of photo-astronomers to this interesting point.

Dordrecht, September 14. C. EASTON.

On the Identification of Habitual Criminals by Finger-Prints.

A PARLIAMENTARY Blue Book on "The Identification of Habitual Criminals," which has recently been issued, reports on *The Finger-Print System*, stated to have been "first suggested, and to some extent applied practically, by Sir William Herschel."

The chairman of the committee appointed by Mr. Asquith, whose report contains the above statement, refers me for his evidence on this point to Mr. Galton's work on "Finger-Prints" (Macmillan and Co., 1892).

My "careful study" of the subject is mentioned there, and an article of mine in NATURE, October 28, 1880 (vol. xxii. p. 605), is referred to. It is correctly indexed in the "Index Medicus" for the year, published in 1881, although Mr. Galton spells and indexes my name incorrectly. That article, I believe, is absolutely the first notice of the subject contained in English literature, and the conclusion I reached therein was that the patterns of the skin-furrows, with their distinctive loops, whorls, and lines, breaking and blending like the junctions in a railway map, were capable of being readily used as a reliable and permanent basis for the "scientific identification of criminals." I conclude my paper with the statement that "There can be no doubt as to the advantage of having, besides their photographs, a nature-copy of the for-ever unchangeable finger-furrows of important criminals."

Sir William Herschel wrote in NATURE, November 25 of the same year, alleging that he had "been taking sign-manuals by means of finger marks for now more than twenty years." It does not yet appear that anything had been published on the subject by that gentleman till my contribution called forth his letter a month afterwards. The collections made by Sir W. Herschel were recently placed in Mr. Galton's hands, and that writer states that "they refer to one or more fingers, and in a few instances to the whole hand, of fifteen different persons." ("Finger-Prints," p. 9.)

It is not stated how many of these had been imprinted prior to my first calling attention to the subject. At present it would seem that Sir. W. Herschel had not accumulated the impressions at a more rapid rate than that of one person in two years! As we are informed in the letter to NATURE, referred to above, that the identification of pensioners had been secured in this way, that the method was in use in all the registration offices of the district, and that "on commitment to gaol, each prisoner had to sign with his finger," I should have expected that a somewhat more extensive collection might have been secured. As priority of publication is generally held to count for something, and as I knew absolutely nothing of Sir W. Herschel's studies, nor ever heard of anyone in India who did, some little evidence on the point of priority would be of interest even now.

Mr. Galton says, of Sir W. Herschel, "He informs me that he submitted, in 1877, a report in semi-official form to the Inspector-General of Gaols, asking to be allowed to extend the process; but no result followed." (p. 28.) A copy of that semi-official report would go far to settle the question of priority, as its date is nearly two years previous to my having noticed the finger-furrows. No reference to them was then to be found in any anatomical work that I could find access to, and no writer on identification had ever thought of them as a means to that end. My interest, like that of Purkenje, arose from a special study of the sense of touch, and I was then lecturing to medical students on the "Physiology of the Senses." Having myopic eyes which enable me to write with ease the Lord's Prayer three times in the space of a sixpence, I soon noticed the unique patterns which the papillary ridges formed. I happened to be studying the prehistoric pottery of Japan at the same time, and became interested in observing that these patterns were similar, but, I thought, finer and more slender than those of the present day, which pointed, I conjectured, to the employment of children in early fictile art. However that may be, my knowledge of the subject had a natural and independent genesis.

The subject of identification by this means has been brought under the notice of the authorities on criminal matters of different countries by me from time to time, and some years before Mr. Galton's work was published, Scotland Yard placed one of its most enlightened officers in communication with me on the subject. Inspector Tunbridge studied the subject with me during a forenoon. Even in 1880, I prepared copper-plate outlines of the two hands, accompanied with instructions as to obtaining finger-prints, and some two chief points on the palm, where the rugæ are characteristic. Sir W. Herschel's letter mentions prints of one finger only as being obtained from prisoners on commitment. On page 79 of the Blue Book mentioned above, "Instructions for taking Finger-Prints" are given for the benefit of prison warders, and the ten fingers are to be printed from, as I have advocated. I may add that I have not the slightest wish to diminish the credit that may be due to Sir W. Herschel. What I wish to point out is that his claim ought to be brought out a little more clearly than has yet been done, either by himself or by Mr. Galton. What precisely did he do, and when? HENRY FAULDS.

The Tetrahedral Carbon Atom.

YOUR reviewer, in his notice of my "Elementary Lessons in Organic Chemistry," takes exception to the statement that the carbon atom has been hypothetically regarded as tetrahedral in shape; he is presumably unacquainted with the criticisms of Lossen (*Berichte* 20, p. 3306) on Wislicenus's memoir, with Wislicenus's reply[1] (*Berichte* 21, p. 581), as well as with the pamphlet of Wunderlich ("Configuration organischer Moleküle," Würzburg, 1886); he need not, however, search "the whole range of stereo-chemical literature" for references of this kind, as there is in the "Handbuch" of V. Meyer and Jacobson, pp. 433-436, a tolerably full discussion as to the ultimate cause of stereo-isomerism in carbon compounds, where it is stated (p. 434) that "the carbon atom may be regarded as a mass of finite extension in space, of any shape, with four points on its surface corresponding to the corners of a regular tetrahedron as the units of affinity."

Most writers on stereo-chemical subjects prefer to speak of the tetrahedral arrangement in space of the four valencies of the carbon atom, rather than of the tetrahedral shape of the carbon atom itself; but if the "valencies" are sufficiently material to have a definite position in space, they may fairly be regarded as parts of the carbon atom, which then becomes of finite size, and *for the purposes of stereo-chemistry* essentially tetrahedral in shape. This form of statement has the merit of simplicity, and is in itself less objectionable than the idea of "valencies" directed towards the corners of a tetrahedron; at the same time, I freely admit that the statement errs on the side of excessive simplicity, and is not what would be adopted before a class of honours students.

It is possible to connect the facts of stereo-isomerism to some extent by a series of separate propositions, and at the same time to avoid any reference to the distribution of the "valencies" in space, or to the finite size of the carbon atom; one of these propositions would be that "two carbon atoms connected by an ethylene linkage are no longer free to rotate round the axis which joins them"; but so soon as an attempt is made to unite these separate statements into one hypothesis, or to assign any reason for the proposition just quoted, it is impossible (as it appears to me) to escape from the dilemma; of the two alternatives, I think most chemists, who have not become blinded by long usage to the gross misuse which the word "valency" has suffered, will prefer to regard the carbon atom as finite in size with four points in it, occupying the corners of a tetrahedron, distinguished in some way beyond the rest as regards the action of chemical affinity.

This idea must be made more definite before the average student can derive much help from it in correlating the facts of

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[1] Wislicenus says "the atom of carbon may possibly resemble very closely regular tetrahedron in shape."

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stereo isomerism; the essential part of it is retained in the assumption that the carbon atom is stereo-chemically to be regarded as tetrahedral in shape. If any student carries away the notion that this is believed to be the actual shape of the atom, there is no more mischief done than in that student's case who gathers the impression that the two carbon atoms united by an ethylene linkage are held together by two pairs of forces which do not act along the line joining the two atoms, but meet at an angle in empty space. G. S. TURPIN.

Huddersfield, September 24.

Careless Writing.

PROF. TILDEN, in his review, published in NATURE, September 20, takes exception to the loose phraseology adopted by writers on chemical subjects. This is, alas! only too common.

For example, in one of the best works on inorganic chemistry, written by a Professor of Chemistry whose writings are characterised by their logical clearness and philosophic reasoning, one may read:—

"When a molecule of hydrogen acts upon a molecule of chlorine to form two molecules of hydrochloric acid gas, 44,000 c. of heat are evolved." Of course, nobody can fail to understand what is meant. But as the words stand, it is certainly one of the most remarkable feats of science, and makes us feel that some happy mortal has succeeded in refining his faculties down to the degree of fineness, popularly ascribed only to a certain species of "demon." F. G. DONNAN.

Holywood, Belfast.

ON THE DOCTRINE OF DISCONTINUITY OF FLUID MOTION, IN CONNECTION WITH THE RESISTANCE AGAINST A SOLID MOVING THROUGH A FLUID.[1]

II.

§ 6. IN every case in which vacuum is formed at an edge of a solid moving in an inviscid incompressible fluid, under pressure constant at all infinitely great distances from the solid, a succession of finite individual vortices is sent from the edge into the liquid, *and the motion is essentially unsteady*. Each individual vortex has a finite endless vacuum for its core instead of the rotationally moving ring of fluid of the Helmholtz vortex ring. But it should be noticed that it would not be rings of vacuum, but bubbles, that would in many cases be first detached from the solid; that by the tumultuous collapse of bubbles they become rings; and that the case in which the collapse of a bubble, in our ideal fluid, could be completed to an annulment of volume, is of necessity infinitely rare; and that the case in which, when a bubble becomes a ring by the meeting of two points of collapsing boundary, there is exactly no circulation through the aperture, is infinitely rare.[2]

§ 7. In the case of our circular disc, it would be circular vortex rings that, if the water were inviscid, would be shed off from its edge when the depth is less than 63 feet. If the depth is very little less than 63 feet these rings would be exceedingly fine, and would follow one another at exceedingly short intervals of time. Thus quite close to the edge there would be something somewhat like Stokes' "rift," but with a rapid succession of vacuum rollers, as it were; and *no slipping* between the portions of the fluid on its two sides.

§ 8. At greater depths than 63 feet, if the water had absolutely no viscosity,[1] the motion would be continuous and irrotational, as described in § 4, text and foot-note: but any degree of viscosity, however slight, would, if the edge were infinitely sharp (instead of having a radius of curvature of 1/2000 of an inch, as has our supposed disc), give rise to a state of motion in its neighbourhood somewhat like to Stokes' rift,[2] "a surface of discontinuity extending some way into the fluid," but with the difference that there is no slip of fluid on fluid. A trail of rotationally moving liquid, a Helmholtz' "vortex sheet" of exceedingly small thickness, is thus left in the wake of the circular edge; which, while becoming thicker as it gets farther from the edge, becomes rolled up in a wildly tumultuous manner, giving the appearance of an irregular crowd of detached circular ring-vortices. This crowd follows the disc at an ever diminishing speed and widens outward farther and farther, and inwards encroaches more on the comparatively undisturbed middle of the wake, as it is left farther and farther behind the disc.

§ 9. Whether as in § 7 for an ideal inviscid incompressible fluid, or as in § 8 for a natural liquid such as water, the "wake," that is to say, the fluid on the rear side of the plane of the disc, as far as it is sensibly affected by the motion of the disc, must be as described in the last sentence of § 8. The rear of the wake is always moving forwards, that is to say, following the disc; but at a continually diminishing speed. Hence, if the disc has been set in motion from rest some finite time, t, ago, the whole wake must be included between the plane from which the disc started, and the plane in which the disc is now, at the time when we are thinking of it. These two planes are at the finite distance, Vt, asunder. In other words the wake extends to some distance less than Vt, rearwards from the disc.

§ 10. The shedding off of vortex rings from the edge of the disc, to follow in its wake at less speed than its own, essentially gives a contribution to negative pressure on the rear side of the disc equal to $\frac{d}{dt}\Sigma\kappa$; where $\Sigma\kappa$ denotes the sum of the circulations of all the coreless ring vortices, or of all the rotationally moving liquid, which have or has left the edge since the beginning of the motion. This, with the commonly assumed velocities of the fluid on the two sides of the rigid plane, seems insufficient to account for the excess of observed pressure above that calculated for a long blade by Lord Rayleigh's formula[3] referred to in my letter to NATURE, "Towards the Efficiency of Sails, &c.," and leaves some correction to be made on those assumed velocities. But the working out of this interesting piece of mathematical hydrokinetics must be deferred for a continuation of the present article in which supposed discontinuity of fluid motion, extending far and wide, as taught by many writers in many scientific papers and text books since Stokes' infinitesimal rift started it in 1847, will be considered. KELVIN.

(*To be continued.*)

MR. SCOTT ELLIOT'S RUWENZORI EXPEDITION.

ABOUT three years ago, in NATURE (November 5, 1891), I gave an account, rescued from an American periodical, of the botanical results, slender enough it is true, but not without interest, of the Emin Relief Ex-

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[1] Continued from p. 525.

[2] The whole subject of the motion of an incompressible inviscid fluid with vacuum on the other side of the whole or any part of its boundary is of surpassing interest. Consider for example an open fixed basin, with water poured into it and left not quite at rest under the influence of gravity; swinging slightly from side to side, let us suppose for example; the water perfectly inviscid, and vapour-less, but may be either cohesional or cohesion-less; and there being perfect vacuum over all its free surface. Very soon it will certainly throw up a drop somewhere: and before very long it will become covered with spin-drift and will thus illustrate Maxwell's important allegation (which I believe to be true though it has been much doubted), that any conservative system must "sooner or later" pass through every possible configuration.

[1] Viscosity is resistance to change of shape in proportion to the speed o the change.

[2] Stokes, "Mathematical and Physical Papers," vol. 1. p. 310.

[3] In lines 9 and 10 of the printed letter (NATURE, Aug. 20, 1894, p. 426), for "something like five or ten," substitute 4·8. I unfortunately had not Lord Rayleigh's formula by me at the time the letter was written.

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